

Klein

October 10, 1957

Dear George:

It was nice to hear from you. I am glad to hear that the program for the symposium is shaping up well. I will deal with Cavalli directly, as I may even be able to see him in six weeks or so. If, unhappily, you have still not heard from him by, say, December 1, I suggest you go ahead with Hayes. I am quite certain that Hayes will be attending the symposium in any case, and there is little chance of 'losing' him by this further delay. He would also probably understand it quite well if Cavalli had been offered a precedence, if only on the grounds of a balanced international representation (viz. U.K., U.S., France, and Italy). But if you possibly can, I suggest you simply hold off a bit longer. I can understand Cavalli's reasons for delay, as his plans for next spring and summer, which are linked to my own, are in turmoil at present.

****Something was bothering me, just as I write this, -- I received a letter from Cavalli, dated August 1, which reached here only three weeks ago, in which he accepted the invitation for Stockholm; it did not occur to me that he might not have been in touch with you, but I am sorry to have overlooked sending that information on. As you may imagine, the organization of my correspondence here is somewhat irregular. At any rate, you can consider this quite definite.

ooOoo

I have been having a rather instructive time here, though at times it is easier to wish for the conveniences and lab. facilities of home. I have a moderately strenuous schedule of lectures, as many out of course as not, but most of my time is with Burnet, where I have been playing with the more systematic application of selective methods to the recombinational genetics of influenza virus, with some rather intriguing results on the fire now. I have also started this week on my first adventures with mammalian cells, addressed to the question whether a single lymphocyte can produce more than one species of antibody. This question is intimately related to some of Burnet's current speculations on the mechanism of tolerance.

After an hour's practise, I found no difficulty in handling single lymphocytes with the same techniques I had learned for Salmonella. That experience, curiously enough, is also the groundwork for our proposed test for antibody, namely the immobilization of bacteria of different flagellar serotypes in the microdroplets containing the cells. Our calculations suggest that we should have no trouble in characterizing the amount of antibody that should be released in our microdroplets, but this has still to be set up. There should be barely time to complete one experimental trial before I leave Oct. 31. If there is time for a reply by then, could you do me the favor of answering the following queries: a) Do you have any easily transplantable ascites tumors that are still capable of giving an antibody reaction? and b) what is your technique of preservation in cold glycerol-- do you think it would be applicable to normal lymphocytes?

After Oct. 31, our present schedule calls for a rapid trip across India and Italy (viz. Milan) to reach home by late Nov. We are feeling rather pressed, however, and may yet revise this to return directly via San Francisco.

With best regards,

Yours sincerely,

P.S. After about Nov. 15, it would be best to forward my mail to Madison rather than Melbourne. If anything urgent comes up, you could try a copy c/o Cavalli, in case we do transit there. J

Joshua Lederberg